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other pre-Darwinian writers, this is a display either of mental density or of something worse, for he understood quite well what I meant, as is seen by his own use of the word 'always' further on.¹⁸

My two main contentions are: *that de Vries's conception of elementary species is inadequate, and that elementary species breed true, not because they are the product of a peculiar kind of variation, called mutation, but because they have been subject to the processes of selection and separation.* These essential points in my criticism have been overlooked by Gager, and he is content to say, with regard to the first one, that nobody, except makers of dictionaries, knows what a species is. With regard to my second contention, he fails entirely to see that it is intimately connected with the first one, and has made no attempt to demonstrate that mutation is capable of producing true breeds *without the help of selection and segregation*, and that the latter two factors *do not play an essential part in de Vries's experiments.* For the rest, he only points to de Vries's definitions of terms, which I reject; he points to the facts represented by the experiments, which I accept, but consider unsatisfactory and incomplete; and he points to the value of the experimental method as the only one that is apt to decide questions of evolution, which I positively deny. Experiments are valuable, but they should be properly understood, and should be correctly explained. The interpretation of his experiments given by de Vries is faulty, although the experiments themselves are indisputable facts; and the fallacy is due to his ignorance of the fundamental laws of evolution, and to his incorrect conception of the term species: with the latter his theory stands and falls.¹⁹

I hope that this will be sufficient, even to Gager, to define my standpoint, and, if any further discussion should be considered necessary, that it will take up the essential points of my views, and not merely repeat the argu-

ments of de Vries. Gager has done only this, in a way which clearly lacks understanding of what I really object to. If he further would consider the rule, not to throw stones at people out of a glass house, and observe the necessary fairness to others, this would make the discussion a more pleasant and profitable one.

A. E. ORTMANN.

CARNEGIE MUSEUM, PITTSBURG,

July 23, 1906.

SPECIAL ARTICLES.

HERBARIUM TYPE SPECIMENS IN PLANT MORPHOLOGY.

THE close relationship existing between the different branches of botany and the dependence of these various branches upon each other make it very important that every precaution should be taken by the workers of each branch to make their specialty as helpful as possible to all other divisions of the subject. With the advancement of each phase of the subject the points of relationship become more prominent and the necessity for the preservation of records, specimens, etc., becomes of greater and greater importance.

Between no two branches of botany is the necessity of cooperation greater than between taxonomy and morphology. The taxonomist has long recognized the importance of type specimens and large herbaria have been brought together and maintained at great expense where these types may be preserved and studied to the best advantage. The morphologist has probably in most cases preserved his microscopic specimens, but in how many cases has the morphologist prepared herbarium specimens of the species on which he is working? This custom may and probably is followed by many workers, but it is also true that many morphologists have not only neglected to preserve type material but in many instances have not even taken the precaution to have their determinations verified by specialists in taxonomy.

If morphological botany is to add anything to our knowledge of taxonomic botany, it appears to the writer that herbarium specimens should be carefully prepared, properly labeled,

¹⁸ *L. c.*, p. 88, foot-note 65: 'Since the process has been recognized and described.'

¹⁹ *SCIENCE*, June 22, 1906, p. 948.

and deposited in centers of botanical research where they may be consulted by future investigators in both taxonomy and morphology.

Some years since the writer made a morphological study of certain members of the family Nymphaeaceæ and among them the northern *Nymphaea advena*. More recently I have made a study of certain tropical species of the same family and among them a species of the genus *Nymphaea*. This species showed such striking resemblances to the well-known species *N. advena*, that it was sent to specialists in taxonomy to verify the determination. The reports from these workers showed a difference of opinion; some claiming that it was a new species, while others claimed that it was a variety. However, the embryology showed certain very marked differences, which may be of sufficient importance to make it a distinct species. Had these two lots of material been studied by different workers, and considered as one species, or by one worker without having the specimens examined by taxonomists, the confusion might have been easily increased rather than diminished.

When we take into consideration the large number of families and genera which are still untouched by the morphologist we must naturally expect that future work will bring to light many new and important facts; and these facts will in turn present certain questions which will make it imperative that certain other species already studied should be restudied in the light of said new facts. It will then be very important that the investigator know positively whether the species in question is the same or merely closely related to the species studied by the first investigator. Johnson,¹ in his studies on Piperales, has recently called attention to the fact that closely related genera may show wide variations in the development of the tapetum, megaspore, embryo-sac and endosperm. From my studies in *Nymphaea* I am inclined to believe that we may also find wider variations between species of the same genera than we have supposed.

Under present conditions two workers in

different localities working upon supposedly the same species may honestly present different results or the second may unintentionally and unjustly give expression to criticism on the results of the first worker.

Would it not be well for the plant morphologists at the next meeting of the American Association for the Advancement of Science to consider methods for cooperation and preservation of types.

MEL. T. COOK.

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THE INTERNATIONAL CATALOGUE OF SCIENTIFIC LITERATURE.

THE first meeting of the International Convention of the International Catalogue of Scientific Literature was held in London, July 25-26, 1905. The supreme control of the catalogue is vested in this body and in beginning the undertaking in 1900 it was agreed that its meetings should be held in London in 1905, in 1910 and thereafter every ten years. The following named delegates were present at the convention:

Austria—Professor Dr. August von Bohm, K. K. Hofbibliothek, Vienna.

Belgium—M. Paul Otlet (Secrétaire-General de l'Office International de Bibliographie, Brussels). M. H. La Fontaine (Directeur de l'Office International de Bibliographie, Brussels).

France—Professor G. Darboux (Secrétaire Perpétuel de l'Institut de France). Dr. J. Deniker (Bibliothécaire du Muséum d'Histoire Naturelle, Paris).

Germany—Professor Dr. O. Uhlworm (Director des Deutschen Regionalbureau).

Greece—His Excellency Mons D. Métaxas (Ministre Plénipotentiaire de S. M. le Roi des Hellènes).

Holland—Professor D. J. Korteweg (University of Amsterdam).

India—Lieutenant-Colonel Prain, I.M.S., F.R.S.

Italy—Cav. Ernesto Mancini (Accademia dei Lincei, Rome). Professor Raffaello Nasini (University of Padua).

Japan—Professor K. Matsubara (University of Tokyo).

Mexico—His Excellency Don Francisco A. de Icaza.

Russia—Professor I. P. Borodin (Imperial Academy of Sciences, St. Petersburg).

¹ Johnson, Johns Hopkins University Circular No. 178. May, 1905.